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Wind forcing of salinity anomalies in the Denmark Strait overflow

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General comments

The authors present salinity measurements from the Ammassalik mooring array downstream of the Denmark Strait and focus on a negative salinity anomaly that occurred in 2004. Several hypotheses are formulated to explain the cause of this anomaly, which are subsequently tested using a high-resolution model. The authors conclude that the anomaly was caused by an increase in southward wind stress along the east coast of Greenland north of the Denmark Strait.

I think that this work is an interesting and valuable contribution to the study of variability in the Denmark Strait Overflow Water. The paper is well written and the figures are for the most part clear and easy to understand. However, I do have a few reservations about the robustness of the model results and would like the authors to consider my comments below. As such, I recommend that the paper must be revised before it can be accepted for publication.

Specific comments:

My major concern regarding this manuscript is the extent to which the results are model dependent or actually represent the real ocean. From Figure 2 it is evident that there is a large discrepancy between observed and modeled salinities at the Ammassalik array. The authors argue that this is of little consequence since salinity anomalies are examined, and show in Figure 5 that the observed and modeled salinity anomaly timeseries are highly correlated. However, the absolute salinity impacts the density structure of the ocean and hence its dynamics. In particular, the observed DSOW plume is banked against the east Greenland slope (Fig. 2a), while the dense model isopycnals are flat (Fig. 2b). From the velocity field (Fig. 4) it is evident that the model's spatial representation of the overflow significantly differs from the observed structure. In the current state of the manuscript, I do not think that the authors have showed convincingly that OCCAM does represent the basic structure of the dense overflow. At the very least, timeseries of observed and modeled DSOW transport must be shown and discussed. Are the mean and variability of the observed and model transports comparable?

Page 1405, line 20
It should be mentioned that also entrainment downstream of the sill can modify the DSOW.

Page 1407, line 23

The Walter et al. (2005) reference is hardly appropriate here. More suitable references are for example Yashayaev (2007, ProgOc) for the Labrador Sea and Pickart et al. (2003, DSR) for the Irminger Sea.

Page 1408, line 14
How can you make an inference about a lack of temporal variability from one snapshot? On a related note, do lateral movements of the DSOW core at Ammassalik occur, and can that account for some portion of the signal in the salinity anomaly timeseries? In particular, it would be nice to see if there is a relationship between the salinity anomaly timeseries and the transport timeseries. If full transport timeseries are not available, velocity records from nearby current meters could perhaps be used. I would think that the authors have examined the salinity and the velocity/transport records in conjunction, and am surprised that this material is not at all presented in the manuscript.

Page 1409, line 24
Have the authors considered a hypothesis H2d, that the 2004 anomaly was caused by a change in the salinity of the source waters originating from salinity anomalies in the inflowing Atlantic water?

Page 1410, line 7
Was the OCCAM model run on a global grid? If not, where are the borders of the grid and what are the boundary conditions?

Page 1412, line 21
The timeseries of observed salinity anomalies at the Ammassalik array and the model salinity anomalies at the sill have a high correlation, and the authors take this as evidence that processes upstream of the sill are responsible for causing the anomaly at the moorings. This is not necessarily true if the model does not represent the mixing/entrainment processes downstream of the sill very well, which you allude to on page 1412, line 7. In that case processes downstream of the sill would have little influence, and the high correlation would necessarily follow.

Page 1414, line 10
The authors rely strongly on the assumption that the model is a fair representation of the real ocean when rejecting hypothesis H2a simply because the model does not contain glacial runoff which does not appear to be necessary to explain the anomalies. It is not unlikely that hypothesis H2a can be rejected, and the authors may be able to justify that using records of observed rates of Greenland ice melt. Studies show an accelerating ice melt, but no apparent anomalies that could explain the 2004 event (see for example van den Broeke et al., 2009, Science, and Velicogna, 2009, GRL).

Page 1417, line 28
Barrier winds, southward winds along the east coast of Greenland, commonly occur in winter (see for example Moore and Renfrew, 2005, JClim). Could this be the reason for the fresh anomalies observed every winter/spring (page 1408, line 3, and page 1408, line 18)? Barrier winds are associated with the passage

of individual low pressure systems and are usually of short duration. Perhaps an unusually large number of barrier wind events in 2004 could, in an integrated sense, be responsible for the observed 6-month salinity anomaly in 2004. I would encourage the authors to include a figure showing the sea level pressure anomaly field over the Nordic seas for this period with vectors of wind anomalies overlaid, which I think would be very enlightening. A climatology of barrier wind events would immediately show if 2004 was an unusual year, but that is probably beyond the scope of this paper. Does the proposed process also explain the 1999 fresh anomaly?

Page 1418, line 9
So observations show that water masses with appropriate salinities are found in the EGC, but are they also present in the model? Also, the Atlantic waters of the EGC are identified by mid-depth salinity maxima – there are other intermediate water masses commonly present within the EGC (for example upper Polar Deep Water and Arctic Intermediate Water in Rudels et al., 2002) that may be more likely culprits of the negative salinity anomaly.

Page 1419, line 12
It does not seem unreasonable that an elevated southward wind stress along the east Greenland coast could increase the transport of the EGC and its contribution to the Denmark Strait overflow relative to other sources. The authors state (without substantiating their claim) that an increased circulation of the Greenland Sea Gyre explains the missing advective time lag between 69°N and 75°N. Does the model show an increased circulation of the Greenland Sea Gyre? Gyres do not seem to respond very quickly to changes in wind forcing and it is likely that spinning up a gyre would be a rather slow process (see for example Spall and Pickart, 2003, JPO). On the other hand, it is perhaps not impossible that an increase in wind stress would generate a response in the form of a barotropic wave traveling southward along the coast of Greenland. This would be fast, and could potentially have an influence on the conditions downstream closer to the Denmark Strait without much delay.

Page 1420, line 25
Can you show a reference demonstrating that the NAO controls the strength of the Greenland Sea Gyre? Otherwise this and the following are somewhat speculative.

Page 1421, line 7
One may argue that even the high-resolution OCCAM model does not accurately represent the EGC (at least not downstream of the Denmark Strait).

Figure 2
Even though it is clearly stated in the text and in the caption, I think that using different color bars is misleading. For direct comparison it would be better to use the same color scale, and if necessary instead add a constant offset to the model fields. I would also like to see markers to indicate where the CTD stations

were made along the section.

Technical corrections:

Page 1404, line 5 (and elsewhere)
The new spelling of Angmagssalik appears to be Ammassalik.

Page 1406, line 6
Regardless of the wind forcing, there is likely not one source that supplies all of the DSOW. It would be better to write that it originates **predominantly** from either of the sources.

Page 1407, line 16
A more common definition of DSOW is water denser than 27.88 (see for example Holliday et al., 2009, JPO, and also Dickson and Brown, 1994), though Dickson et al. (2008, the ASOF book) may have used 27.85.

Page 1412, line 18 (and elsewhere)
To increase readability, it may be better to repeat which hypothesis is to be tested in the title of each section.

Page 1420, line 25
I would modify the start of the sentence to "This means **that**..."

Figure 1
The Iceland Sea is not a pathway.

Figure 10
There is a dot missing after (b).